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Q. XXVIII. Account of the Airs extracted from different Kinds of Waters; with Thoughts on the Salubrity of Air at different Places. In a Letter from the Abbé Fontana, Director of the Cabinet of Natural History belonging to his Royal Highness the Grand Duke of Tuscany, to Joseph Priestley, LL.D. F. R. S.

Read April 16, 1779.

I TAKE the liberty of sending you an account of some experiments which I made at Paris in the years 1777 and 1778 on the air extracted from various kinds of waters: some of the principal of which I thought proper to transcribe, that you might make use of them if you think that they are at all useful, and likewise lay them before the Royal Society.

I have extracted the air from the water of a well by means of common fire. The water was then made to boil in a large mattrafs of tin, which had a long tube of the same metal, which being bent into two different directions was with its extremity immersed in a tub of cold water. The mattrafs and its tube were intirely
filled

filled with water: the air which came out of it was received into three different vessels. The air of the first vessel, by being shaken in water, was diminished a little; the air of the second was diminished of half its bulk, or rather more; and the air of the third vessel was diminished exceedingly. The *residuum*s of air that remained unabsorbed were more or less phlogisticated.

Another time I obtained almost entirely fixed air, excepting a little which remained unabsorbed by water, and was partly phlogisticated.

A third time the air of the water of a well, obtained as above, was made to pass through mercury into a tube anointed with oil of tartar, and it occasioned a crystallization just like that which the purest fixed air is used to do.

A fourth time I impregnated with this air a quantity of common water, which absorbed its own bulk of it, and became by these means acidulous, exactly like water with the purest fixed air. This water turned the tincture of turnsole red, and precipitated the lime in lime water. Another time a light was successively extinguished, and a bird died instantly in this air.

The water of the river Seine, filtrated through sand, as it is drunk at Paris, was treated in the same manner as the water of the well. The air extracted from that water was half absorbed by water, when shaken in it; the remainder,

remainder, when treated with the taste of nitrous air, gave II-4, II+1^(a); when the common atmospheric air treated with the same nitrous air gave II-4, II+8. It was therefore sensibly better than the atmospheric air, which, during three years of experiments made at Paris, I have constantly found to be inferior to the air of the Seine water, extracted as above.

Having repeated the experiment, I received the air into two different receivers. The first of which, by being shaken in water, was diminished in the proportion of ten to seven; and by the test of nitrous air gave II-14, II+1, III+1, when the common air gave II-12, II+6, III+6.

The second quantity of air was diminished in the proportion of three to one; and when examined by the test of nitrous air gave II±0, III±0. From whence it may be concluded, that the first air was better than the atmospheric air; whereas the second was worse, and mixed with much fixed air.

Being in doubt whether the tin vessel employed in the experiment above mentioned might not alter the nature of the air, &c. I made use of glass vessels. Having therefore filled one of these vessels, having a long neck bent in two directions, with the Seine water, I

(a) See p. 343. for an explanation of this measure.

obtained some air which seemed not sensibly diminished when shaken in water. Having introduced one measure and 37 parts of this air into the tube used to try the diminutions, it gave with the nitrous air $I+19$, $I+48$, when the same quantity of nitrous and atmospheric air gave $II+26$, $II+6$: it is therefore certain, that the air extracted from Seine water is purer than common air.

Another time I extracted, in the same manner, and from the same water, the air; one measure and 24 parts of which being introduced into the tube, &c. and shaken, was reduced to one measure – 31 parts, that is, one fifth of it was absorbed. Treated with the nitrous air it gave $I-4$, when equal measures of common and nitrous air gave $I=0$: it was therefore better than common air.

A third time I extracted the air, in the manner above mentioned, from the water of the river Seine, contained in three mattraffes: this air was about one twenty-eighth of the bulk of the water, and it gave with the test of nitrous air $II-14$, $II-9$, $III-9$; when the common air mixed with nitrous air, as usual, gave $II-14$, $II+8$, $III+8$. It is therefore clear, that the air extracted from the Seine water, by the action of fire in glass vessels, is much better than common air, or than the air which is extracted from the same water when boiled in tin vessels.

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Another time I filled a glass retort, which had a long and doubly bent neck, with Seine water. The water weighed about three pounds. The air that came out of it lost a quarter of its bulk by being shaken in water; and afterwards being tried with nitrous air it gave II-16, II-16, III-16, when common and nitrous air gave II-12, II+12.

This experiment being repeated, the air was diminished of one quarter by being shaken in water. One measure - 16 parts of this air introduced into the measuring tube gave II-32, II-2, when common air and the nitrous gave II-28, II+4.

The water d'Argeuil at Paris is considered as very pure. I filled the tin vessel above mentioned with it, and received the air that came out of it into three vessels. Being shaken in water, the first of them was diminished one fifth; the second, three fourths; and the third $\frac{15}{16}$ by the operation in water. A light burned with a flame, more luminous than in common air, in the first air after it had been shaken in water. This air being tried with the nitrous air gave II-10, II-10, III-10. The second gave II-10, II-17, III-30, when common and nitrous air gave II-2, II+14, III+14. The third air, before it was shaken in water, crystallized with the oil of tartar like fixed air. An equal bulk of it was absorbed by

water, which by this mixture became acidulous; it precipitated the lime in lime water, extinguished a light several times, and killed an animal instantly. It is therefore partly fixed air, and partly air which is not only better than common air, but likewise than that extracted from Seine water, even when this last has been boiled in glass vessels.

The experiments being repeated with the same water of Arqueil, but in glass vessels, the air obtained, after being shaken in water, was much better than that obtained from the same matter, when boiled in vessels of tin.

I have also extracted the air from distilled water in glass vessels, and having shaken one measure – 32 parts of it in water, it was reduced to one measure – 35 parts. With the test of nitrous air it gave 1–6, when equal parts of common and nitrous air gave 1–2, which shewed that it was better than common air.

I extracted the air again in the manner above described; but it was not sensibly diminished when shaken in water. Two measures – 49 parts of it, with the test of nitrous air, gave 1–2, 1+8, when common air, &c. gave 1+1, 1+18: It is therefore better than common air.

I extracted the air from a great quantity of distilled water in the usual manner, and found that it did not sen-

fibly diminish in water. With nitrous air it gave II-14, II-25, II+25, when common and the same nitrous air gave II-14, II+10, III+10; consequently it was dephlogisticated air, *viz.* purer than the air of the Seine and Arqueil, which are much better than common air.

I had the curiosity to try, whether any difference would arise from boiling distilled water in a matrafs of tin instead of glass vessels; and found, that the first air was diminished one tenth by being shaken in water, and afterwards with the nitrous air gave II-13, II-16, III-18; when common air gave II-12, II+8; which shews that it was dephlogisticated air, but not so good as that extracted from the same water when boiled in glass vessels. The second quantity of air was not sensibly diminished in water, and with nitrous air gave II-13, II-20, III-30; that is, it was more dephlogisticated than the first.

The air extracted from distilled water is to that extracted from the water of the river Seine as 13 to 32 nearly; whence distilled water does not give more air than one sixtieth of its bulk: but as the air extracted from the water of the Seine is half fixed air, it may be concluded, that the quantity of respirable air produced by both kinds of waters is nearly the same, and that they only differ a little in purity. It is however true, that other ex-

periments have shewn me that water in general absorbs about twice as much of dephlogisticated as of common air; for which reason, I think, that the respirable air of Seine water is rather less than that of distilled water. Accordingly I have found, that Seine water, after it has been boiled for a long time, absorbs in forty days about one fourteenth of its own bulk of dephlogisticated air, when in the same length of time it does not absorb more than one twenty-eighth of common air. This seems to be an experiment of very great consequence, and is much worth notice; especially as it discovers a new characteristic by which dephlogisticated air may be distinguished from common air; and shews, that water absorbs a greater quantity of those kinds of air, which contain a less quantity of phlogiston.

It must however be observed, that it is impossible to determine exactly the quantity of air that is extracted from vessels filled with water, by means of fire; because a portion of the air is absorbed by the water of the tub in the act of its coming forth. It will certainly be more exact to receive the air in vessels immersed in quicksilver; but then there are many other inconveniencies to encounter.

It may be almost superfluous to mention, that the above related experiments are very useful in explaining

the reason why some kinds of water have a peculiar sharp taste more than others; and especially why some of them precipitate the lime in lime water, rendering it a calcareous earth, and change the tincture of turnsole into a red colour, as I have generally experienced with the well waters at Paris. We may also explain from hence, why some kind of waters can dissolve iron, and keep it in dissolution without deposition; whereas other kinds of water are incapable of doing it, at least do it much less than the purest distilled water. This is soon discovered by boiling the water, which will then deposit the iron which before was dissolved.

It will be sufficient, for the present, to mention, that I have not only extracted from waters the different kinds of air they contained naturally, but have likewise made various experiments upon waters deprived of air, which, being exposed, have again imbibed the atmospherical air, as I hinted above. I have determined the quantity and quality of those airs. In general, I may say, that distilled water, deprived of air, imbibes again an equal quantity of air of the same kind as that it had lost, and that in less than fifty days. Other kinds of water do the same, but with this difference, *viz.* that the air they absorb, after being boiled, is better than that they have lost; and in this particular they come very near to the nature of distilled water itself.

If the waters deprived of air are exposed to common air held in receivers in contact with quicksilver, the air which remains unabsorbed is so much more phlogisticated as a less quantity of it remains in the receiver. This experiment deserves consideration.

By means of pure water, especially distilled water, common air may be changed into dephlogisticated air, that is, into air much more salubrious than the best common air which we breathe; and this, for what I know, is the only means of meliorating common air: for all the artificial methods (great numbers of which I have tried) have proved either useless or noxious, but never such as promised to be of any great utility to human kind.

Though I have long thought of applying those experiments to some use for the purposes of life, the want of time and a proper apparatus has hitherto hindered my doing any thing; I now begin to be in hopes I shall be able to do something. In the mean time I think it of some importance to have it known, that water not only possesses the property of diminishing the noxious part of tainted air, but has also the power, and that in a very high degree, of dephlogisticating common air; which must certainly be one of the methods by which nature keeps the atmosphere in a state constantly fit to support animal life, it being certain, that the water in various circumstances

circumstances must lose either a part or the whole of that air which it hath absorbed from the atmosphere.

It may with some reason be suspected, that in my experiments of extracting the air from water by the action of fire, the air might be considerably altered by the vapour of the water itself. As this difficulty was of some force, I endeavoured to remove it in the following manner. I introduced into a tube, through water, a quantity of common air of known goodness, and I caused the steam of water boiling in a matrafs, from which the air had been previously extracted, to pass through it. The heat of the steam sometimes made the water occupy above five times the space it did when cold; yet the air so treated was not at all altered by it, as appeared by the test of nitrous air. The event of the experiment, although repeated various times, was constantly the same.

I must observe, lastly, that having once caused the air of boiling water to pass into receivers filled with, and standing in, quicksilver, I found that the air was better than usual. I have observed the same thing when I have caused the air to go through distilled water into receivers filled with it: which observation, if the event of the experiment is constantly the same, induces me to believe, that the air loses some of its good properties by going through water not very pure; or, which seems to be
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rather more probable, that a quantity of air less good is, by the action of the vapours and the heat, extricated from that impure water, and is mixed with the air that comes out of the matrafs; from whence this air is debased.

I must not omit to mention a new character of equal importance with that which distinguishes the dephlogisticated from the common air. This new character has been equally unknown, and deserves the attention of philosophers, because at the same time it discovers a new property of the atmospherical air, which I should never have suspected if experience had not offered it to me.

I have found, that common air shaken in water, instead of being diminished is sensibly increased in its bulk. The increased space is in proportion to the time the air is shaken in water, and it begins to be sensible even from the beginning, that is, after a few seconds. This augmentation I have sometimes brought to be one twelfth of the bulk of the air, and even more: it must, however, be confessed, that I met with great variety in the experiments of this kind made at different times. After that the bulk of the air shaken in water is increased to a certain degree, it then begins to decrease continually; and, in proportion to this decrease, the air becomes gradually less good. When the experiment is tried in close vessels, the diminution

nution cannot be observed; but I shall reserve, for another opportunity, to speak of the laws and causes of those diminutions and augmentations, and the differences observed between common and other kinds of air, and when the experiments are tried in water, &c.

For the present I shall only mention, that if the dephlogisticated air be shaken in the tube, in the manner above mentioned, not only it does not increase its bulk, but it begins to diminish from the very beginning of the operation, and it continually loses more and more of its bulk, and with its bulk of its purity.

This last mentioned property of the dephlogisticated air seems to shew, that this is a fluid much different from common air, because it has its peculiar properties by which it differs from common air not from more to less only, but entirely; as is shewn by the property this fluid has of being absorbed by water; whereas common air receives an increase of bulk and elasticity by being shaken in water.

All that I have been saying above, in order to give an idea of my method, and the words I use to express the diminutions made by the mixture of nitrous air and other kinds of respirable air, is not sufficient to obtain results constant and certain, so as to deduce any consequences from them. Even after that all the elements are
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corrected, and all the causes of error hitherto unknown or neglected by the most diligent observers, are avoided (which causes sensibly alter this kind of experiments) it is absolutely necessary to follow always a constant and equal method, not only in the act of introducing the various kinds of air into the tube, but also after the mixing of the two kinds of air. The least variation of circumstances causes very great variations in the results of the experiments, and these variations of circumstances are so minute that I never saw any of the persons that observed my experiments, who could discover them, although apprized of my design. The neglect alone of this uniformity of operation may occasion an error of from 20 to 50 parts and upwards in the experiment with common air; but with dephlogisticated air the error is incomparably greater, so much that the same quality of air a moment after may decompose even a double quantity of nitrous air; so that the purest common air would appear to be noxious, and phlogisticated air; and the dephlogisticated air would appear less good, and even noxious: for, by the test of nitrous air, it might appear little different from a mixture of dephlogisticated and phlogisticated air.

I shall take another opportunity to speak of all the particulars relative to my method; but for the present I

must mention, that, after using all the cautions I am master of; the greatest error in the diminutions is not greater than one sixtieth of the common air introduced in the tube: so that, after having made five or six experiments successively, the probable error is so small that it may be safely neglected. But, if one chose to operate upon a quantity of air nine or ten times greater than that I commonly use, the error could not be 1000th part of the quantity of the air, which quantity would not be more than a few cubic inches. In the description of my method I shall also mention the means by which I obtain nitrous air of a nearly equal and constant goodness, and in what manner I can refer my experiments to some constant standard. For want of this method it is that we are not certain of the observations made about the salubrity of common air in different places, and that no tables of its changes have been made.

I have not the least hesitation in asserting, that the experiments made to ascertain the salubrity of the atmospheric air in various places, in different countries and situations, mentioned by several authors, are not to be depended upon; because the method they used was far from being exact, the elements or ingredients for the experiment were unknown and uncertain, and the results very different from one another.

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When all the errors are corrected it will be found, that the difference between the air of one country and that of another, at different times, is much less than what is commonly believed, and that the great differences found by various observers are owing to the fallacious effects of uncertain methods. This I advance from experience: for when I was in the same error, I found very great differences between the results of the experiments of this nature which ought to have been similar; which diversities I attributed to myself rather than to the method I then used. At Paris I examined the air of different places at the same time, and especially of those situations where it was most probable to meet with infected air, because those places abounded with putrid substances and impure exhalations; but the differences I observed were very small, and much less than what could have been suspected, for they hardly arrived to one fiftieth of the air in the tube. Having taken the air of the hill called Mont Valerien at the height of about 500 feet above the level of Paris, and compared it with the air of Paris taken at the same time and treated alike; I found the former to be hardly one thirtieth better than the latter. In London I have observed almost the same. The air of Islington and that of London suffered an equal diminution by the mixture of nitrous air; yet the

air of Islington is esteemed to be much better. I have examined the air of London, taken at different heights; (for instance, in the street, at the second floor, and at the top of the adjoining houses) and have found it to be of the same quality. Having taken the air at the iron gallery of St. Paul's cupola at the height of 313 feet above the ground, and likewise the air of the stone gallery which is 202 feet below the other; and having compared these two quantities of air with that of the street adjoining; I found, that there was scarce any sensible difference between them, although taken at such different heights.

In this experiment a circumstance is to be considered, which must have contributed to render the above mentioned differences more sensible; this is, the agitation of the air of the cupola, for there was felt a pretty brisk wind upon it, which I observed to be stronger and stronger the higher I ascended; whereas in the street, and indeed in all the streets I passed through, there was no sensible wind to be felt. This experiment was made at four in the afternoon, the weather being clear. The quicksilver in the barometer at that time was 28,6 inches high, and FAHRENHEIT's thermometer stood at 54°. After having related all these circumstances, it will be necessary to give the mean result of all the various experiments

periments made upon each of those quantities of air treated after my method with the nitrous air. The air of the street gave II-13, II+6; the air of the stone gallery, which was 202 feet high, gave II-14, II+5; and the air of the iron gallery, which was 313 feet high, gave II-14, II+5. The results of the two last experiments are exactly the same; and that of the first is hardly at all different from them. Mr. CAVALLO, who has shewn the literary world his ability in examining nature, assisted me in those experiments, so that a mistake can hardly be suspected. From this we clearly see, how little the experiments hitherto published, about the differences of common air, are to be depended upon. In general I find, that the air changes from one time to another; so that the differences between them are far greater than those of the airs of different countries, or different heights; for instance, I have found that the air of London, in the months of September, October, and November, 1778, when treated with the nitrous air, gave II-6, II+15, which is a mean result of many experiments which differed very little from each other. The 26th day of November last, I found the air for the first time much better, for it gave II-12, II+12; but the 14th of February last, the air gave
II-18,

II-18, II+7; from whence it appears, that the air of this 14th of February was better than it had been for six months before. There can be no doubt of the accuracy of the experiments, because I compared the air taken at different times with that which I had first used in the month of September, and which I had preserved in dry glass bottles accurately stopped. Now if the formulæ expressed above are compared together, it will be found, that the difference between the first terms is of twelve parts, and that between the latter of seven; that is, of one tenth and one twenty-fourth of the whole quantity of air: which are much greater differences than those mentioned above. Notwithstanding this, I could not perceive any particular change of health, or facility of breathing, arising from those changes of the salubrity of the atmospherical air; and I am informed, that no particular diseases appeared which could indicate any remarkable change of air.

Nature is not so partial as we commonly believe. She has not only given us an air almost equally good every where and at every time, but has allowed us a certain latitude or a power of living and being in health in qualities of air which differ to a certain degree. By this I do not mean to deny the existence of certain kinds of noxious

air in some particular places; but only say, that in general the air is good every where, and that the small differences are not to be feared so much as some people would make us believe. Nor do I mean to speak here of those vapours and other bodies which are accidentally joined to the common air in particular places, but do not change its nature and intrinsic property. This state of the air cannot be known by the test of nitrous air, and those vapours are to be considered in the same manner as we should consider so many particles of arsenic swimming in the atmosphere. In this case it is the arsenic, and not the degenerated air, that would kill the animals who ventured to breathe it.

In this place, therefore, I do not mean to speak of those changes which do not immediately alter the nature of the air itself. The other states of that fluid are of another kind, and they are not to be examined by means of nitrous or inflammable air (the uses of which last, I shall shew on another occasion). The same thing may be said of those vapours or particles which may be good for respiration, and do not change the nature of the air. Some vegetables, for instance, can diffuse through the air such exhalations as may be of real use to the animal œconomy when they
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are breathed for a long time, or imbibed by the pores of the skin. I remember to have often put various flowers, as roses, pinks, &c. in vessels full of common air confined by quicksilver, where I left them for several hours; after which time I found, that the air was not at all altered, but that various animals seemed to breathe it very well, notwithstanding that the flowers filled the greatest part of the vessels. On the contrary, I have found, that the vapours arising from lime slacked in water, either do not alter the air at all, or very little; though when breathed with the air they occasion the death of animals.

I would not have any body suppose, that I think it of little importance to know the goodness of the atmospherical air, and the changes it undergoes. On the contrary, I believe it to be a very useful inquiry for mankind, because we do not yet know how far one kind of air more than another may contribute to a perfect state of health; nor at what time small differences may become very considerable, when one continues to breathe the same kind of air for whole years, especially in some kind of diseases. An exact method of examining the goodness of common air may even be useful to posterity, in order to ascertain whether our atmosphere degenerates in a length of time. This curious inquiry, together
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with the method, &c. are the production of this eighteenth century; and our descendants must have some gratitude for the philosophers who found out, as well as for those who improved it. If our ancestors had known and transmitted it to us, we should, perhaps, at present be able to judge of one of the greatest changes of our globe, of a change which very nearly interests human life.

